

Sedimentation in the Delta and Suisun Bay

David H Schoellhamer

Final Selection Panel Review

Proposal Title

#0255: Sedimentation in the Delta and Suisun Bay

Funding:

Do not fund

This proposal has two components. One is the continuation of a long-term monitoring effort which has and would provide important information on the sediment in the Delta. The project provides for the acquisition, storage, organization, analysis, interpretation and dissemination of the collected data. These aspects of the project are well established. Some evolutionary improvement in the instrumentation is proposed.

The second is the use of previous and new data to test a set of geomorphic hypotheses. Apart from dealing with these quite general hypotheses and the proposed channel mapping and flow modeling, there is little new scientific research proposed. This project is well aligned with the elements of the CALFED priority areas as validated in part by previous funding for earlier phases of this work. The PI's comment stating that the reviewers did not fully characterize the publications record of the proposers is correct. However, that review comment was not a significant factor in the rating of the proposal or the TSP's decision to recommend not to fund. This proposal is essentially a continuation of previous work. Reviewers questioned both the nature of the hypotheses made and whether or not they can be tested within the project's timeframe. They also questioned the budget, which appears large and top heavy with senior investigators. Although the investigators are likely to do an excellent job in carrying out this work, when compared to other higher ranked projects, it was deemed a lower priority for the Science Program. Accordingly, it is the

#0255: Sedimentation in the Delta and Suisun Bay

Final Selection Panel Review

Selection Panel's assessment that the TSP rating of adequate is correct and that this project should not be funded at this time.

Public Comments

The following public comments were received for this proposal.

Subject: Science Program proposal evaluation and SFEWS

I am writing to make you aware of the following conflicting actions of the CALFED Science Program which seriously undermine Program goals:

The CALFED Science Program promotes publication in their peer-reviewed journal San Francisco Estuary and Watershed Science;

The CALFED Science Program proposal evaluation process did not consider San Francisco Estuary and Watershed Science to be a peer-reviewed journal.

Publication in San Francisco Estuary and Watershed Science:
The Science Program funds San Francisco Estuary and Watershed Science (SFEWS), promotes publication in it, and has made it a peer-reviewed journal in order to encourage scientists to submit manuscripts. SFEWS must be perceived by the scientific community as being a peer-reviewed journal in order to attract high quality manuscripts and for authors to be credited with producing peer reviewed publications by promotion and proposal review panels.

Proposal evaluation:
Randal Dinehart and I submitted a proposal to continue our project 'Sedimentation in the Delta and Suisun Bay' to the recent Science Program PSP. Technical reviewer 3 commented on the Project publication list that 'only 2 are published papers in widely read, peer-reviewed outlets. I think that this is a major weakness of the proposal.' (Two articles listed in the proposal as submitted to Water Resources Research and Journal of Hydrology recently have been accepted). The reviewer clearly gave credit for papers in Journal of Hydrology and Estuarine, Coastal and Shelf Science and not two papers in SFEWS (and peer-reviewed papers in IAHS and Elsevier books). The Technical Synthesis Panel stated 'Only 2 references on their work are in peer-reviewed journals' and 'The panel was very concerned about the past lack of peer-reviewed publications by the researchers'.

It is clear that CALFED Science Program's own proposal evaluation process gave no credit for publication in SFEWS. The admonitions of two Chief Scientists and the CALFED Science Program to publish in SFEWS would appear to have been best left unheeded. All facets of the Science Program should support and reward publication in SFEWS. If the Science Program truly supports peer-reviewed science and publication in SFEWS, then the proposal evaluation process was erroneous.

Sincerely,

David Schoellhamer
Research Hydrologist, U.S. Geological Survey

attachment: PDF file with formatting and links
(See attached file: Schoellhamer PSP SFEWS.pdf)

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Information on Suspended-sediment transport in San Francisco Bay and Delta
Continuous Monitoring Data from San Francisco Bay
USGS Publications Related to Continuous Monitoring of San Francisco Bay

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David Schoellhamer
U.S. Geological Survey
Placer Hall
6000 J Street
Sacramento, CA 95819-6129
(916) 278-3126, FAX: (916) 278-3071
dschoell@usgs.gov

Technical Synthesis Panel Review

Proposal Title

#0255: Sedimentation in the Delta and Suisun Bay

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

Summary: This is essentially continuation of a long-term, mostly monitoring project of sediment transport and deposition patterns in the Delta. The authors propose to monitor sediment loads, transports, distribution, use state of the art technology (ADCPs) to map channels and channel flow velocities in Delta region, and describe sediment movement through delta channels. There little new research in this proposal. The authors and reviewers all agreed that monitoring of sediment transport is one of the few available methods for evaluating trends that are important to the Ecosystem Restoration Program. The authors propose a number of hypotheses they will test with the program. The hypotheses appear rather weak, and it is not clear that these hypotheses will adequately tested in this proposal. Some hypotheses depend upon collaboration with other scientists measuring fish behavior or mercury concentrations, which are briefly referred to but not explicitly described here. It is likely that most of these hypotheses could be tested by these authors now or in the future. Some hypotheses include: • The sediment yield of the Sacramento River will continue to decrease. • Increasing depositional area of the Delta will increase fraction of inflowing sediment trapped in the Delta. Depositional area would be increased by restoration actions and un-intentional levee breaks. • Operational changes, will alter sediment

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Technical Synthesis Panel Review

transport and deposition. • Transport of sediment-associated contaminants is strongly linked to sediment transport. The project is very feasible. The PIs are experienced, and have the tools and capability to run this project. They are highly organized. Products will be readily available to users, although this aspect could be improved over the current outlets of agency reports. Only 2 references on their work are in peer-reviewed journals. It's clear that this is an important project which enjoyed unanimous support by all 3 reviewers. I did wonder whether at the end of 3 years, USGS will have predictive model of sediment transport, and could now reduce the monitoring and develop a predictive. The question with monitoring always is, how much is enough? Especially given this budget is high, nearly \$2 million. The budget for the co-PI seems high! (3 years of salary support, 600K!). Perhaps the sediment monitoring activity should be paid by the USGS! The proposal was well written, and the information provided indicated the importance of their earlier work, and conceptual understanding of sediment dynamics in the system. I would like to have seen more definable endpoints. The channel mapping and flow modeling is just such certainly a new and important objective.

Additional Comments:

Summary: This is essentially continuation of a long-term, mostly monitoring project of sediment transport and deposition patterns in the Delta. The authors propose to monitor sediment loads, transports, distribution, use state of the art technology (ADCPs) to map channels and channel flow velocities in Delta region, and describe sediment movement through delta channels. There little new research in this proposal. The authors and reviewers all agreed that monitoring of sediment transport is one of the few available methods for evaluating trends that are important to the Ecosystem Restoration Program. The authors propose a number of hypotheses they will test with the program. The hypotheses appear rather weak, and it is not clear that these hypotheses will adequately tested in this proposal. Some hypotheses depend upon collaboration with other scientists measuring fish behavior or mercury

Technical Synthesis Panel Review

concentrations, which are briefly referred to but not explicitly described here. It is likely that most of these hypotheses could be tested by these authors now or in the future. Some hypotheses include: • The sediment yield of the Sacramento River will continue to decrease. • Increasing depositional area of the Delta will increase fraction of inflowing sediment trapped in the Delta. Depositional area would be increased by restoration actions and un-intentional levee breaks. • Operational changes, will alter sediment transport and deposition. • Transport of sediment-associated contaminants is strongly linked to sediment transport. The project is very feasible. The PIs are experienced, and have the tools and capability to run this project. They are highly organized. Products will be readily available to users, although this aspect could be improved over the current outlets of agency reports Only 2 references on their work are in peer-reviewed journals. It's clear that this is an important project which enjoyed unanimous support by all 3 reviewers. I did wonder whether at the end of 3 years, USGS will have predictive model of sediment transport, and could now reduce the monitoring and develop a predictive. The question with monitoring always is, how much is enough? Especially given this budget is high, nearly \$2 million. The budget for the co-PI seems high! (3 years of salary support, 600K!). Perhaps the sediment monitoring activity should be paid by the USGS! The proposal was well written, and the information provided indicated the importance of their earlier work, and conceptual understanding of sediment dynamics in the system. I would like to have seen more definable endpoints. The channel mapping and flow modeling is just such certainly a new and important objective.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Sedimentation in the Delta and Suisun Bay

The panel noted that most activities that were proposed are monitoring and not new research.

Technical Synthesis Panel Review

Hypotheses are considered rather weak and how they will be tested is not clear (e.g., sediment yield in Sacramento River will continue to decrease).

The project team is very experienced, the work is very feasible, and the proposal well-written. A conceptual model was evident in the proposal. The panel suggested that the researchers focus on using their data collected in the past to develop a quantitative model of the system, rather than focus solely on additional monitoring and the continuation of data collection and methodology refinements.

The best part of the proposal was the proposed channel mapping and flow modeling which would be new research and was well justified. The rest of the proposal is monitoring and may not be suitable for the Science Program.

The panel was very concerned about the past lack of peer-reviewed publications by the researchers, and was concerned about the prospect that no peer-reviewed publications would result from this work.

The panel felt that the proposed work was very expensive.

Rating: adequate

Technical Review #1

proposal title: Sedimentation in the Delta and Suisun Bay

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	Yes .
Rating	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	<p>Continued monitoring of sediment transport and sediment deposition patterns in the Delta is justified because this is one of the few available methods for quantitatively evaluating related trends that are important to the goals of the Ecosystem Restoration Program.</p> <p>The conceptual model is clearly stated and it explains the basis for the proposed work.</p> <p>The scale of the project is necessary to meet the goals and objectives of the work because it includes the important sediment inputs to the bay, and provides a means of evaluating deposition and patterns of sediment movement within the bay.</p>
Rating	excellent

Technical Review #1

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	As demonstrated by the author's apparent past success, the project appears to be well designed, appropriate and feasible with respect to implementing the data collection and mapping components of the work. The resulting information will add significantly to the base of knowledge. The proposed approach is a novel method for collecting the indicated data using state-of-the-art equipment. Although the resulting data and information will be useful in testing the specific hypotheses that are listed in Section 2), it is doubtful that the proposed workplan is actually adequate to fully test these hypotheses. The information that results from the study will, however, be very useful to other scientists who are studying these and related issues in the Delta.
Rating	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The approach to collecting, handling, storing and disseminating the information is well documented and appears to be technically feasible. The project has a good likelihood of success. As described above, the scale of the project is consistent with the objectives because it considers the key sediment inflows to the Delta. The authors' past experience indicates that successful implementation of the work is within their grasp.
Rating	

Technical Review #1

	excellent
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Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	This work is essentially a continuation of a previously implemented sediment monitoring program that appears to have been successful. There are plans to interpret certain aspects of the data.
Rating	very good

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The data resulting from this program would be value, and the contribution to larger data management systems is relevant and has been considered. Interpretive outcomes appear to be somewhat limited, but the project will provide a considerable amount of data that is interpretable in evaluating specific issues that relate to sediment loads and sedimentation patterns in the Delta.
Rating	excellent

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Technical Review #1

Comments	The authors appear to have a good track record of performance, based on their peer reviewed publications. Based on their past performance on this work, they also appear to be well qualified and have the infrastructure and support necessary to accomplish the work.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	Although the \$1.9M budget seems, on initial consideration to be quite large, it is probably justified considering the amount of effort, the required equipment, and the three-year period over which the project will be implemented.
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	Continued collection and monitoring of sediment loads to the Delta and sedimentation patterns within the Delta is important to understanding a broad range of issues that are important to the CALFED Ecosystem Restoration Program. In my opinion, this project would make a valuable contribution to the available base of information on these issues.
Rating	excellent

Technical Review #2

proposal title: Sedimentation in the Delta and Suisun Bay

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	A nicely written proposal, clearly laying out the purpose, scope, and methods of this ongoing work. Continued monitoring of flow and sediment transport through the Delta area is absolutely essential to support restoration efforts.
Rating	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	The need for the ongoing monitoring is clearly explained. The connection between the channel flow mapping and ecosystem issues is also made. The conceptual model presented essentially summarizes findings to date which, collectively, represent important steps forward in understanding the flow and sediment dynamics.
Rating	excellent

Technical Review #2

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	Consistency in monitoring is essential. Existing sites must be maintained unless they become obviously irrelevant and new sites should be added only if it is possible to maintain them. The proposed work is consistent with this. Channel flow mapping gets at questions of how sediment moves through the delta, supporting conceptual and computational models for channel dynamics and restoration. A table of all monitoring locations and activities, conducted by all parties, not just the USGS, is needed to effectively evaluate the program.
Rating	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The standard methods are well tested and the new and evolving methods undergo rigorous evaluation. Maintaining a physical sampling program in parallel with the surrogate sampling measures remains a priority.
Rating	excellent

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	
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Technical Review #2

	Data checking and dissemination appear to be appropriate. USGS sets the standard in these areas.
Rating	excellent

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	Data are essential; effective interpretations of data have been made and communicated.
Rating	excellent

Additional Comments

Comments	It would be more appropriate to have a separate proposal for the basic monitoring of water and sediment flux and the channel flow mapping. Although done by the same agency, these activities serve different purposes and evaluation of the work would be more accurate if done separately. That said, it appears that both activities are highly effective and merit continued support. The monitoring data are essential and the channel mapping work is innovative and appears to be leading to very useful observations of how sediment moves through the delta, including temporal and spatial detail that can help diagnose problems and plan restoration practice.
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Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	Who (else) are you gonna call?
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Technical Review #2

Rating	excellent
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Budget

Is the budget reasonable and adequate for the work proposed?

Comments	\$2 million does give one pause. This budget is too large and complex, and the activities are too numerous and integrated with other measurement and monitoring activities, for an outside reviewer to effectively comment on. Clearly, the importance of the data and the cost of collecting it require an ongoing dialog and specific management attention by someone with an intimate knowledge of the players and the activities. The question of who is paying for what and whether the allocation is equitable requires far more information. Whatever happens, please don't let bureaucratic interactions or fluctuations in funding interrupt the essential monitoring function provided by the USGS. "not applicable" means "not able to judge"
Rating	not applicable

Overall

Provide a brief explanation of your summary rating.

Comments	Essential data. Innovative development and application of new sampling methodologies.
Rating	excellent

Technical Review #3

proposal title: Sedimentation in the Delta and Suisun Bay

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The goals of this project are, essentially, to continue the work that has been ongoing by the group on measuring and modeling sediment fluxes, and to continue to develop advanced methods for sediment measurement (optical backscattering, etc.). This research falls within the context of sedimentation within the delta, and the geomorphic and hydraulic changes that follow. These goals are of interest to CALFED, and the goals of developing advanced methods for sediment transport measuring are particularly important and timely. Measuring sediment fluxes in large rivers is only viable through these types of technology, and so continuing in their development is a profoundly important goal.
Rating	very good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	I have mixed feelings on the issue of justification. This project will continue development of a measurement system. I find this to be extremely
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Technical Review #3

	important, but it is not necessarily justifiable as novel research. A question that emerges from their justification is "How much measurement is enough?" That is, is the refinement of measurement techniques a never-ending research agenda? Do the authors have a specific goal for measurement technology that they can hold up as an end-point goal? If not, then how can one assess their success over the lifespan of the project?
Rating	good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The approach is very sound; they will continue to leverage off of the existing data and field campaigns that they have been conducting, but will continue to refine their measurement techniques. This project, in conjunction with their previous projects will likely lead to continued novel developments of sediment/hydraulic measurement technology.
Rating	excellent

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	For the proposed work, this is the team to do the work. They have the know-how, the equipment, the infrastructure, and the personnel to do the project. Their proposal is based on years of experience, and thus has the highest likelihood of success.
Rating	excellent

Technical Review #3

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	The monitoring and data collection they have proposed is sound and solid, and is well thought out.
Rating	excellent

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	I was the least impressed with the products the authors propose generating. I do not think that reports and conference presentations are sufficient output for projects of this scale and scope. These authors need to be getting this information and technology out to the scientific community in peer-reviewed outlets. This is not just to get academics reading it, but these data and methods need to be through rigorous peer review, and their reference list does not indicate that they are going through this process. I appreciate that USGS has their own publishing practices, but many leaders in USGS continue to publish their science in peer-reviewed journal outlets. If the authors do not do this, the broader scientific community will not be aware of their findings, and their research will not be sufficiently externally reviewed. For instance, I count 30 references in their list with an "*", but only 2 are published papers in widely read, peer-reviewed outlets. I think that this is a major weakness of the proposal.
Rating	fair

Additional Comments

Comments	<p>I think that this is exactly the kind of work that the USGS should be in the business of doing, and thus I am strongly supportive of funding this type of work. Research like this by USGS personnel on the Missouri River has been fundamental in developing federal policies for the management of the river. Were it not for technology developed by USGS personnel for large rivers, there would be essentially no science available from which to make decisions. If USGS doesn't do this kind of work in development of measurement technology, it won't get developed.</p> <p>That said, I think that the funding agency should encourage the authors of this proposal to make a concerted effort to making their technology more widely available. They mention that post-processing of these types of data is the limiting factor; if they are going to continue to refine the post-processing of data, is there a way that they could make the rest of us aware of how they suggest doing it? They mention "custom software." To whom will this software be available? Again, if it is only for a limited group, then the process will never be rigorously reviewed, and thus be of limited value.</p>
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Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	This is a tough project to pull off, and I think that few groups could take it on. This is a group that can do the work though.
Rating	excellent

Technical Review #3

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget is realistic, and the authors (based on their years of experience) will know what it takes to do the work; I have to say that it hurts to see the overhead charged by USGS, but I appreciate that this is what it takes to keep them going.
Rating	excellent

Overall

Provide a brief explanation of your summary rating.

Comments	<p>I strongly encourage that this project be funded; the continued development and refinement of the data collection and analysis is fundamentally too important to not fund this proposal.</p> <p>However, I also strongly encourage the authors to place greater emphasis on getting their research more broadly reviewed and distributed. What they are doing is important and interesting, but not enough scientists are seeing it.</p>
Rating	excellent

